

been consumed, not in overcoming resistance to the poleward motion of the water, but in overcoming the resistance to eastward motion. The energy consumed is deflecting energy, not impelling energy.

According to Dr. Carpenter's theory the pound of water has in virtue of gravity only six foot-pounds of energy to carry it from the equator to lat. 60° against all the resistances to its poleward motion; but it so happens that before the water reaches lat. 60° no less than 9,025 foot-pounds of energy is consumed in overcoming resistance to eastward motion. But if it requires 9,025 foot-pounds of energy to overcome the resistance to eastward motion, how is it possible that Dr. Carpenter's six foot-pounds of energy can overcome the resistance to the poleward motion? The velocity of the motion of the water polewards is as great as, if not greater than, the velocity of the motion eastwards, consequently the resistance to the motion of the water poleward must be as great as the resistance to the motion eastward. But if so, then the work of the resistances to poleward motion is 1,500 times greater than the work of gravity. The work of gravity being only six foot-pounds, whereas the work of the resistances is 9,025 foot-pounds.

One of two things must therefore follow as a necessary consequence: (1) either the work of the resistances to poleward motion is 1,500 times greater than the work of gravity, or (2) the work of the resistances to poleward motion is 1,500 times less than the work of the resistances to eastward motion. But either conclusion is equally fatal to the gravitation theory.

It seems to me that until the advocates of this theory manage to escape from this dilemma, it is needless to argue further on the matter. For, unless it can be shown that the work of the resistances is not greater than the work of gravity, the much disputed question as to whether or not difference of specific gravity can be the cause of a general interchange of equatorial and polar water must be regarded as finally settled in the negative.

I cannot help thinking but that Mr. Ferrel is misled by his supposed analogy between a slope produced by the influence of the attraction of the moon and that produced by difference of specific gravity. Although a slope of 9 feet in a quadrant resulting from difference of specific gravity is insufficient to produce motion of the water, nevertheless, the sea will easily regain its level after the attractive force of the moon is withdrawn, even though the height to which the surface of the ocean is raised may not exceed a single inch. The reason of the difference in the two cases must be obvious to any one who will reflect on the matter. I have already in my paper in the Phil. Mag. for Oct. 1871 alluded to this reason, and will have occasion again to refer to it at greater length.

I may notice that by a typographical error in my article the velocity of rotation at lat. 60° was stated to be 773 feet per second instead of 763 feet per second.

Edinburgh, July 18

JAMES CROLL

The Melbourne Telescope

MR. ELLERY has been so good as to send me an enlargement of the lunar photograph taken with the great Melbourne telescope, to which you allude at p. 228, No. 142 of your Journal. This picture, Mr. Ellery tells me, was taken on the second evening of trial; it is very beautiful, although not so critically sharp as several I have obtained with my Newtonian equatorial of 13 in. aperture, and a little more than 10 ft. focal length. This sharpness, however, is a mere question of the shadiness of the atmosphere; and I feel persuaded that pictures will be taken with the Melbourne telescope far surpassing any hitherto procured. In my telescope the focal image varies from 1 in. to $1\frac{1}{8}$ in. in diameter, according to the distance of the moon from the earth. The primary picture of the Melbourne telescope (an enlargement of which has been sent to me) is $3\frac{1}{8}$ in. in diameter; hence the structure of the collodion and minute defects in it are of much less importance than when smaller instruments are used.

The employment of the great Melbourne telescope for astronomical photography cannot fail to be of great advantage to astronomy, and I should be very glad to see a similar instrument at work in England, notwithstanding its too much abused climate.

WARREN DE LA RUE

P.S.—As soon as the Melbourne picture has been mounted and protected, I will place it in the Astronomical Society's rooms for inspection.

On the Rigidity of the Earth and the Liquidity of Lavas.

IN his letter upon the Rigidity of the Earth and the Liquidity of Lavas in the number of NATURE for July 11, Dr. Sterry Hunt has replied to my challenge to propose an explanation of the connection between mountain ranges and trains of volcanoes consistent with a rigid globe, other than that to which I refer it, viz. the production of fusion through a diminution of pressure due to the partial support of the mountains by the lateral thrust which has upraised them. Dr. Hunt suggests that liquefaction may take place beneath such ranges, through increased pressure promoting the liquefaction of the water-impregnated mass; and quotes the late Archdeacon Pratt as maintaining the existence of a greater pressure beneath mountain ranges.

In reply, I may be allowed to ask Dr. Hunt for a reference to such an expression of Pratt's opinion. I cannot call to mind any passage of his to that effect. The result of his calculations of the attraction of the Himalayas upon the plumb-line showed that they do not attract so much as they ought to do, and he explained this by supposing a deficiency of matter beneath the mountains. His own explanation of the phenomena, as given in the fourth edition of his Theory of the Earth, finished very shortly before his lamented death, is, "that the varieties we see in mountains and plains and ocean beds in the earth's surface, have arisen from the earth having been once a fluid or semifluid mass, and that in solidifying the mass has contracted unequally, so as to form hollows where the contraction has been greatest, into which water flowed and formed seas and oceans, and to leave high table-lands and mountain-ranges where the contraction has been less." (He speaks here of contraction in the vertical direction.) A geologist will, I suppose, receive this as a very incomplete explanation; but the material point is that the Archdeacon was led to adopt it because he had discovered a deficiency of matter beneath the Himalayas. This seems incompatible with Dr. Hunt's view (both with regard to Pratt's opinion on the subject, and with regard to the fact itself) that there is an increased pressure beneath mountain ranges.

It will now appear that my "speculation" upon the origin of volcanic action was suggested by the proved deficiency of matter, and consequently probable diminution of pressure; and not that the idea of diminished pressure was invented to account for volcanic action. I have merely proposed a connection between lateral pressure and diminished density which seems most natural, namely, that the same pressure which upraised the mountains continues partially to support them. And I cannot see how it can do otherwise. For the abutments of the mountains having approached by contraction of the crust, cannot again recede without expansion, which cannot take place. Dr. Hunt's view of the liquefaction of lavas, to my mind, requires explanation. Admitting that pressure promotes aqueous liquefaction in heated rocks, when rocks so liquefied began to rise in a volcanic mass, would they not be brought under diminished pressure, and would they not become immediately solidified, so that they could not reach the surface in a fluid state?

Moreover, since liquefaction, according to this view, is increased by pressure, the interior parts of the earth being under greater pressure than the more superficial strata, ought, at least to that depth where water is present, to be more liquid, and this would be incompatible with the supposition of a rigid globe which Dr. Hunt favours.

Harlton, Cambridge

O. FISHER

The Method of Least Squares

WILL you allow me to call the attention of Mr. J. W. L. Glaisher to the following sentence from Encke, Berliner Jahrbuch 1853, p. 311. "Ich werde mir deshalb erlauben, völlig dem Gange den Lagrange genommen hat folgend, wie könnte man sich erdreisten, bei der ungemeinen Klarheit, Einfachheit und Tiefe des grossen Meisters, eine irgend bedeutende Änderung vorzunehmen, den Theil der Abhandlung hier wiederzugeben, welcher den Beweis für das arithmetische Mittel enthält, und selbst Sätze, die im Grunde schon die Methode der kleinsten Quadrate in sich begreifen." Also to article 17, Corollary, of the Memoir of Lagrange.

This is not the place to discuss the doctrine of the Method of Least Squares; but I may say that in my judgment the method rests on the assumption of the principle of the arithmetical mean, an assumption which is justified by an universal experience.

Having made this assumption, the rule that the sum of the squares of the remaining errors is a minimum follows very simply: *vide* the late Memoir of Hansen, art. 3.

With regard to the practical application of the Method of Least Squares, I think the whole honour of its introduction belongs to Gauss. The rules which he and his scholar Encke have given for the application of the method, and for executing the numerical operations, are so complete and perfect that but little more can be desired.

Washington, July 4

ASAPH HALL

Solar Rainbow

ON the 10th instant at about half-past seven in the evening I saw part of a well-defined rainbow about 5° west of the zenith, the convexity of the bow towards the setting sun, which at the time was about 3° above the horizon. Light clouds were passing beneath the bow. There was no rain.

Brighton, July 15

GEORGE DINNOW

Hive Bees *v.* Mechanism

I HAVE never followed Huber through his wonderful researches into the astounding working proceedings of hive bees—that elder Huber, who, by the way, aided by so admirable a spouse, brought his researches to so successful an issue, notwithstanding his blindness. Hence my excuse, if what I attempt to describe as being original to my own sense of observation, prove not so to others. I think it is conceded universally that amongst other leguminous plants peas have ever been secure from cross fertilisation, one variety with the other, in so far as natural influences, insect agency, &c., are concerned. Our stocks of garden peas, though known to run weedy and grow inferior when cultivated too long upon one kind of soil, very rarely, if ever, sport or vary as other plants placed in juxtaposition of species, especially varieties, are known to do. So decidedly has this fact been confirmed, that invariably sweet peas, even when it is desirable to grow them true to name, are sown in rows, side by side, whites, scarlets, blues, &c., with the utmost impunity. And this is wholly owing to the fact that the floral envelopes are so securely wrapped around the pistil and the stamens, that these parts cannot be reached without the exertion of more power than the strength bees and similar winged insects are supposed ordinarily to possess; unless, indeed, mechanism be called to their aid—a science in itself, but which, nevertheless, has been resorted to in the instance to which I would direct attention. Here then the hive-bees methodically bare the stamens by sheer mechanical force, and rob each of its load of pollen by sense of touch alone. And this they do in this wise. Alighting on the *ala* or “wings” of each bloom, they first of all press their heads up under the base of that part of the papilionaceous corolla called the “standard,” or *vexillum*, and extract what nectar they find. Then, with their little heads firmly pressed therein, and holding fast by the four fore legs, they exert their power, thus artificially contrived, by treading down both the aforesaid wings and the “keel,” or *carina*, which so securely envelopes the sexual organs, that they protrude, so that the anthers are laid bare, when they generally rub the pollen off the stamens on to their hairy bellies, only occasionally using one hinder leg to aid them. It will be seen that they do this with their hinder legs and body, at a time when their heads are entirely hidden from view. I have tested these parts in regard to the pressure needed to disclose the pollen thus, and find that a pressure of half to three quarter ounce is necessary; and computing the weight of an individual bee to be about the sixteenth of an ounce, we see what an amount of power must be exerted in this hind-before, or blindfold manner, by these interesting little creatures.

I should add, however, though I have been a selector of sweet peas for more than a dozen years in other parts of the country, as the selection sold by some seedsmen with my signature attached confirms, I nevertheless have not previously believed in any power possessed by insect agency to thus destroy selections. Here, at Valentines, however, being only seven miles from London, it would appear that the bees, like town sparrows, are unusually “wise in their generation,” and that, owing to scarcity of honey-yielding materials, they are driven to such wonderful feats as I have explained.

WILLIAM EARLEY

The Gardens, Valentines

The Red Rocks

If the peroxide of iron was deposited (as in the Swedish lakes) as brown hydrous peroxide, and if long boiling in the laboratory may be considered analogous to evaporation in an inland sea, then it would appear from the following extract from Watts's “Dictionary of Chemistry” that there is no difficulty in accounting for the colour of the red rocks:—

“A remarkable insoluble modification of ferric hydrate is produced by boiling the ordinary yellow hydrate to $2\text{Fe}_2\text{O}_3 \cdot 3\text{H}_2\text{O}$ (precipitated from the chloride by ammonia) in water for seven or eight hours. The colour then changes from ochre-yellow to brick-red, and the hydrate thus altered is scarcely acted upon by strong boiling nitric acid, and but very slowly by hydrochloric acid. In acetic acid, or dilute nitric or hydrochloric acid it dissolves, forming a red liquid, which is clear by transmitted, but turbid by reflected light; is precipitated by the smallest quantity of an alkali salt or a sulphate; and on addition of strong nitric or hydrochloric acid, yields a red granular precipitate, which redissolves on diluting the liquid with water.”

The change of colour from brown to red is readily obtained by boiling the hydrate in a flask for several hours, as described above. The change is gradual, and before becoming finally red the precipitate is of a chocolate colour, corresponding with that sometimes observed in the red sandstones.

Small pieces of white sandstone, introduced into the flask during the boiling, are of course coloured red, and resemble red sandstone when taken out and dried.

R. D. P.

Instantaneousness of Lightning

DURING a recent night thunderstorm I got out my colour-top, with the usual disc of so-called primary colours arranged to blend into grey or white on rotation, in order to show to my children the instantaneousness of the lightning, and that by its light the disc would, as I had no doubt, appear stationary in one or several successive positions according to the character of the flash, as it does by the light of an ordinary electric spark from a Leyden jar or induction coil. On trying the experiment, however, by turning the disc (about forty times in a second) at a window in a dark room opposite to the cloud in and from which the discharges were taking place, I found that this was only very partially the case. When the direct stroke was actually visible, or only slightly veiled by cloud, the effect I looked for was produced, the bands of colour standing out clear and apparently motionless; but at other times during the apparently (to the eye) prolonged flash, the colours blended so as to indicate a continuous fainter light in addition to the occasional instantaneous appearance of definite colour and form due to the intermittent light of the discharge. Of course I satisfied myself that there was no other light to account for this, the night and the room being very dark in the intervals of the flashes, and I repeated the experiment in another night storm (on the 11th) with just the same result. The effect appears to be due to the retention of light in the cloud by phosphorescence, and, so far as I can find on inquiry, does not seem to have been noticed before.

If my view is correct, it would explain the fact that distant lightning at night, when no cloud is in sight, is apparently so much sharper than when nearer.

A propos to the subject, a letter to the *Times* describing the storm of the 11th inst., speaks of the lightning “flickering with a perceptible rustle”—a curious instance of transference of impression from one sense to another, the visual sensation of flickering being exactly analogous to the auditory sensation of rustling.

B. W. SMITH

Hampstead, July 20

Severe Thunderstorm

A HEAVY thunderstorm passed over this neighbourhood this afternoon. One of the flashes was followed by a smart “snap” like that of the discharge of a large Leyden jar, or the explosion of a heavy percussion cap. The interval between this noise and the crash of the thunder was about half or three-quarters of a second. I have not unfrequently noticed a hissing noise immediately preceding a violent clap of thunder, but never anything so distinctly resembling the sound of the ordinary electric spark.

ALEX. BEAZELEY

Willesden Lane, Kilburn, July 23